



How do assessed values affect the transaction prices of homes? ☆☆☆

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ABSTRACT

Property taxes are commonly levied as a percentage of a home's assessed value (AV). AVs should affect home prices through opposing channels. An unexpected increase in AV implies higher tax payments, which should hurt a home's selling price (tax channel). On the other hand, the increase should have a positive effect since AVs serve as reference prices (anchoring channel). In a quasi-experimental setting exploiting geographic variations in AV-publication dates and reassessment frequencies, we find that a higher AV leads to a lower transaction price, indicating that the tax channel prevails. Disentangling the aggregate effect, we document that empirically, the anchoring channel does not play a major role. Our results thus suggest an exacerbation of previously documented inequities caused by taxation.

1. Introduction

For many governments worldwide, property taxes are a significant source of income. In 2022, for instance, US homeowners paid more than 710 billion USD in property taxes, accounting for almost 35% of state and local tax revenue.¹ In practice, authorities tax each home by a proportion of its assessed value (AV), a fair value estimate generated by a local official. AVs are thus fundamental for calculating individual tax bills and simultaneously provide updated market value estimates for the underlying homes. This ambiguous role of AVs poses a challenge for homeowners and policymakers, as it remains unclear how the information contained in AVs is incorporated into trading prices, not only when

buying or selling a home but also when evaluating the fairness of the currently employed taxation scheme. The latter becomes particularly important, as recent literature documents substantial assessment errors leading to poorer households and racial minority residents being systematically disadvantaged (Avenancio-León and Howard, 2022; Berry, 2021). Yet, despite the significance of AVs for housing markets and the equitable redistribution of wealth, their relationship to trading prices remains largely unexplored.

In this paper, we investigate the causal impact of AVs on homes' transaction prices.² This effect should be driven by AVs' unexpected (random) components through three channels. First, once a home's

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¹ US Census Bureau, Quarterly Summary of State and Local Government Tax Revenue. Last accessed on October 19, 2023 from <https://www.census.gov/data/tables/2022/econ/qtax/historical.Q4.html>.

² In the most technical sense, the term "assessed value" could also describe the taxable *share* of the estimated market value provided by the local assessor (i.e., the taxable assessed value). We depart from this technical definition and use the term "AV" throughout the manuscript to describe the assessor's total market value estimate. Hence, the reassessments investigated in this work relate to "reassessments with reappraisals".

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updated AV is sent to homeowners, it can serve as a potential reference price, e.g., during price negotiations. An unexpectedly higher AV should, therefore, increase the trading price of the corresponding home (anchoring channel). Second, an unexpected increase in the AV implies a higher future property tax burden, which should negatively affect a home's trading price (tax channel) (Oates, 1969). Third, before the publication of a new AV, individuals face assessment uncertainty, which they try to avoid as the possibility of paying excessive taxes due to a large and positive evaluation shock outweighs the potential utility gain from a negative shock of the same magnitude. We control for assessment uncertainty using a tax-adjusted assessment volatility measure.

We identify whether the anchoring or the tax channel prevails by employing a difference-in-differences (DD) regression setup that exploits the timing at which updated AVs are sent to homeowners and geographic variation in the frequencies by which homes are reassessed. Our results document that the tax channel prevails; an unexpectedly higher AV causes a decline in the corresponding home's transaction price. Exploring the quantitative importance of the tax and the anchoring channel, our results suggest that empirically, the anchoring effect does not play a major role, i.e., our treatment effect essentially measures the pure tax effect. This finding indicates an amplification of inequity through distorted assessments, as owners of over-assessed homes must pay higher taxes and sell their homes at a significantly lower price.

Our empirical analysis uses a unique dataset that stems from representative data on both historical tax records and individual transactions of homes, as well as hand-collected AV-publication dates. We focus on single-family homes in New York State for four reasons. First, within each reassessing municipality, updated AVs are collectively mailed to homeowners until a particular deadline, allowing us to compare pre- and post-publication periods. Second, reassessment frequencies vary at the municipal level, so homes sold during tax years without reassessment serve as a control group. Third, AVs are not forecasts but have to reflect home values in the past. Fourth, unlike in California, for example, AVs are not subject to post-transaction adjustments. Instead, AVs are generated by a local assessor collectively. Taken together, these characteristics enable the employment of a DD setting that allows for causal inference.

To identify the effect of AVs on transaction prices within a DD framework, a measure other than raw sales prices is necessary. This need arises as homes can be heterogeneously affected through the same channel, even within the same treated municipality. We introduce a measure that focusses on the relative difference between sale price and AV that overcomes this problem. For instance, regardless of which of the two channels prevails, homes with unexpectedly low AVs face opposing price changes compared to homes with unexpectedly high AVs.

Our empirical results, which align with the predictions from a theoretical model, document that the random components of the respective AVs affect transaction prices negatively, in line with a prevailing tax channel. In particular, the relative sales-price-AV distance increases by 1.1 percentage points after updated AVs are published. The result is not only statistically robust, e.g., to the inclusion of different local and temporal fixed effects, but also economically meaningful. A back-of-the-envelope calculation shows that the documented effect translates into a change in the counterfactual sales price of an average home of about one percent. Consistent with the tax channel, units associated with higher effective tax rates are more strongly affected than units with lower rates. Right after AV publication, we document a relatively smaller treatment effect that is converging to a new equilibrium after 3–4 months, suggesting that right after publication, anchoring is at least partly counteracting the tax channel. The empirical results confirm an additional prediction from our theoretical model, which implies price discounts that increase with the uncertainty about future assessments. An event study, carried out to provide simple and intuitive evidence

on unexpected AV increases and sales prices, confirms the documented DD results in direction as well as order of magnitude.

The results of this paper have several practical implications for homeowners as well as policymakers. First, the prevailing tax channel amplifies inequity, as homeowners with unexpectedly high AVs not only have to pay higher taxes, but also suffer from a substantial price discount on their home. This amplification is particularly important, as recent literature documents that over-assessment systematically disadvantages minority residents (Avenancio-León and Howard, 2022). Hence, policymakers have additional incentives to increase the precision of AVs, for instance, by shortening the time interval between reassessments, by collecting additional, high-quality data, or by improving their models, for instance by employing machine learning methods. Importantly, Avenancio-León and Howard (2022) show that assessment caps can also address this issue by allowing for higher house price growth in such areas, as well as by limiting assessment errors. Finally, the double-punishment documented in this paper could be avoided by policy changes that prevent misspecified AVs from entering the pricing equation of buyers, for instance, by setting the initial AV after purchase to the respective transaction price (as, e.g., in California).

Second, while homeowners planning to stay in their home should generally contest AVs if they are unexpectedly high, in order to avoid unnecessarily large tax payments, they should contest high AVs even when selling the underlying home, in order to avoid the documented price discount. In contrast, homeowners with unexpectedly low AVs should not contest their home's assessment, as the gains from a higher reference price are outweighed by tax considerations. Lastly, our results document that market participants value rational tax implications over a (behavioral) anchoring heuristic, thus providing empirical support for rational choice models.

This work contributes to a growing strand of literature on the effect of property taxation on trading prices. Exploiting a trial tax in China, Bai et al. (2014) and Du and Zhang (2015) show that introducing a property tax can have a negative or zero effect on price growth. Similarly, Elinder and Persson (2017) find that only extremely high-valued homes respond with price declines to an unexpected tax cut in Sweden. Further work, such as Hilber (2017), Livy (2018), Palmon and Smith (1998), and Wassmer (1993), documents that tax rate changes are negatively capitalized in sales prices. While this literature focuses on the tax rate and the introduction of property tax systems, the tax base (here, the AV) has received less attention. This is surprising, given that tax bases can change in different directions within the same district and thus potentially affect prices in opposite directions, even for neighboring homes. This is in sharp contrast to changes in the tax rate, which should lead to a collective adjustment of prices of all treated homes in the same direction. We thus contribute to the taxation literature by showing that the tax base itself affects transaction prices and thus revealing an additional taxation effect that is heterogeneous even within the same treated location. Additionally, this paper provides theoretical and empirical support for a novel, homogeneous factor, that negatively affects house prices in a given area during the pre-assessment period, the local tax-adjusted assessment uncertainty. We thus complement the result of Anderson (2012), who finds that market value assessments lead to large idiosyncratic risk to tax payments, by showing that assessment risk also affects transaction prices.

We further contribute to the literature on inequity in property taxation by uncovering price distortion as an additional source of inequitable outcomes. So far, other work such as Allen and Dare (2002), Goolsby (1997), Hodge et al. (2017), and Sirmans et al. (2008), focus on inequity in tax payments, the effect of the reassessment frequency on the variability of assessment rates (Hou et al., 2025), and the relationship between assessments and taxes (Ihlanfeldt and Rodgers, 2022; Kim and Hou, 2025; McMillen and Singh, 2023), separately. We join these strands of literature by showing that misspecified assessments can even be capable of distorting sales prices themselves. As our results

indicate that AVs negatively impact sales prices, we document a double punishment for homeowners with unjustified high AVs. Not only do such owners have to pay an excessive amount of tax, but they also suffer from a price discount when selling their home.

By investigating AVs as a potential anchor for buyers and sellers of homes, we contribute to the literature that underpins the importance of reference quantities in the housing market. So far, several anchoring phenomena have been investigated, revealing that anchors play a highly important role in housing markets. Northcraft and Neale (1987) document that even professional real estate agents adjust their appraisals towards a randomized listing price. Genesove and Mayer (2001) show that homeowners consider the initial purchase price as a reference point when they are selling their homes. Andersen et al. (2022) estimate a structural model of listing decisions and identify the nominal purchase price as a reference point for homeowners. Fischer et al. (2021) document that realized returns of homes traded in close neighborhoods have increased predictive power for future prices once they are publicly recorded, supporting the finding of Szumilo (2021) that even price increases from unobserved renovations can spill over to neighboring properties. Similarly, Bailey et al. (2018) show that individuals even rely on the house price growth experienced by distant friends when making their buy or rent decision.

In the context of anchoring on AVs, Jones (2020) shows that homeowners confronted with an increase in their AV have a higher propensity to contest their home's assessment, which can be linked to loss aversion. Considering the AVs as an anchor for valuations instead, the evidence provided by the current literature is mixed. Cypher and Hansz (2003) do not find any anchoring on assessed values in an experimental setting. In contrast, Levy et al. (2016) find homeowners in New Zealand to be influenced by values that are used for property taxation, but are not necessarily market value estimates. While these studies investigate AVs primarily as an anchor, this study expands this view by studying the interplay between tax and anchoring channels in a quasi-experimental framework.

The remainder of this paper is structured as follows. In the next section, we motivate the channels through which AVs are likely to influence trading prices within a theoretical framework. Section 3 describes the empirical approach followed in this paper. Our data is discussed in Section 4. Results are presented in Section 5. In Section 6, we discuss identification in more detail. Section 7 concludes. The Online Appendix provides details on New York's property tax calendar, formal derivations for the theoretical framework, the data cleaning and construction of key variables, details on the event study, as well as various robustness results.

2. Theoretical considerations

In this section, we first present crucial features of the New York State property tax system in order to establish the practical foundations for the model presented afterwards. This model first motivates both anchoring and tax channel, and second, illustrates the implications of both channels for the causal impact of AVs on transaction prices.

2.1. Determination of AVs

Within each municipality in New York State, AVs are generated by a local official and are based on recent sales of comparable properties and, if homes are reassessed, sent to homeowners collectively around a particular date.³ The frequencies in which homes are reassessed are

³ Comparability is determined by the assessor based on property features such as size, age, and location. The actual set of characteristics that is collected for each home varies across municipalities, however. Using the data collected, the assessor uses mass appraisal methodologies to generate initial estimates wherever sufficient transactions are available. The assessor is allowed to revise these estimates, if necessary, before finalizing the assessment roll.

determined by the municipal governments, and range from annual to once in several decades. If a municipal government reassesses homes for the upcoming tax year, usually following a scheduled reassessment program, preliminary AVs are sent to homeowners as "assessment disclosure notices" on or around a given publication date, T .⁴ At this date, usually the "taxable status date", the condition of a home is also fixed for the upcoming reassessment.⁵ If a given municipality does not reassess homes in the given tax year, property taxes are based on AV of the most recent year.⁶ The frequencies vary across municipalities, but to receive financial support through the "state aid for cyclical reassessments" program, complete reassessments with reappraisals have to be conducted at least every four years. The existence of pre- and post-publication periods as well as years without reassessment in some municipalities yield the key ingredients for the DD analysis described in Section 3.

Apart from AVs, taxes paid by homeowners depend on multiple factors, such as the local tax rate, the local assessment ratio, and individual exemptions. The effective tax rate, τ , can vary across individual homeowners, even within the same neighborhood. Since the total property tax levy is fixed for a given tax year, the final tax payments also depend on the relative AV of a home compared to all other AVs in a given jurisdiction. For example, if AVs increased by 6% due to a reassessment, owners experiencing only a 5% increase would have to pay fewer taxes than the prior year. In contrast, owners with a 7% AV increase would have to pay more than before (assuming the same total tax levy for both years).⁷ An extended description of the New York State property tax system, based on the state's property tax calendar, is provided in Appendix A. The following section provides a theoretical model that illustrates the effect of AVs on transaction prices of homes.

2.2. A simple model

We consider a model similar to the closed-form framework in Landvoigt et al. (2015), extended by an uncertain second period and property taxation, but with divisible housing. A representative household maximizes expected lifetime utility, V , over two periods by choosing between (numéraire) consumption c_t ($t = 1, 2$) and units of (divisible) housing stock $n > 0$.

⁴ McMillen and Singh (2023) find mean reversion in assessment rates for properties in Cook County, Illinois. In our data, we do, however, not find evidence for mean-reversion. The correlation between changes in consecutive reassessments in New York City, where homes are reassessed on an annual basis, is as low as 0.0131. If ever, there is positive autocorrelation, but also this effect is so small that changes in AVs should basically come as surprises to homeowners.

⁵ A condition might be "vacant" as also described in the official tax calendar, which can be found at the New York State Department of Taxation and Finance website: <https://www.tax.ny.gov/pit/property/learn/proptaxcal.htm> last accessed on October 19, 2023.

⁶ It is possible, that in some tax years, local assessors of some municipalities adjust AVs collectively by the same factor, e.g., to adjust for inflation or general market movements. As such collective changes should not alter the relative pricing in a given area, and collective price changes can be filtered out by fixed effects, we use such observations as an additional control group.

⁷ Additionally, two policies limit the increase of tax payments. First, in New York City and Nassau County, the taxable share of the fair market values can increase maximally by 6% per year or 20% over five years, which might limit the effect of the tax channel. In the robustness Section Appendix F, we show that our results are robust to the exclusion of these areas. Second, in most areas, such as counties and cities outside NYC, the annual increase in the districts' total tax levy is capped by the minimum of either 2% or the CPI inflation rate. Tax levy limits do not rule out significant individual changes in tax payments, however. For instance, a substantial AV increase relative to other properties in the municipality would still lead to a substantial increase in tax payments, even if the total tax levy remained constant.

In the first period, the household buys a home at a price that is determined by $P(n) = \hat{p}n$, in which $\hat{p} > 0$ is the price per unit of a home and n is the number of units of the home, which can, e.g., be measured in square meters. In the second period, the household pays property taxes as a fraction of the home's realized AV, $\tau AV(n)$. However, the tax payments are uncertain, for instance, because the assessor is uncertain about the true pricing model. For tractability, we assume that the valuation shock, ε , is uniformly distributed between $-q$ and q with $q > 0$, such that the realized per-unit AV, \hat{p} , is determined by $\hat{p} = \mathbb{E}[\hat{p}] + \varepsilon$. Note that parameter q has a direct uncertainty interpretation, as it follows from the uniform distribution that the volatility of the assessment shock, ε , is given by $\sigma = q/\sqrt{12}$. Parameter τ is the household's effective tax rate.

For simplicity, further assume that the household is a home buyer and holds initial wealth, $W > 0$, that is used to pay for the home, property taxes, as well as consumption in both periods. Assuming a log-additive utility function and that wealth can be frictionlessly transferred to the second period, the household solves the optimization problem

$$\max_{c_1, c_2, n} V(c_1, c_2, n) = \ln(c_1) + \beta \mathbb{E}[\ln(c_2)] + (1 + \beta)\theta \ln(n) \quad (1)$$

$$s.t. \quad W = c_1 + c_2 + n(\hat{p} + \tau \hat{p}) \quad \text{and} \quad (2)$$

$$\hat{p} = \mathbb{E}[\hat{p}] + \varepsilon \quad \varepsilon \sim \text{Uniform}(-q, q), \quad (3)$$

in which $0 < \beta < 1$ is a time preference parameter and $\theta > 0$ determines the importance of housing relative to consumption. Optimizing for c_1 , c_2 , and n , the first order conditions imply

$$\frac{1}{c_1} = \beta \mathbb{E} \left[\frac{1}{c_2} \right] \quad \text{and} \quad (4)$$

$$\frac{(1 + \beta)\theta}{n} = \beta \mathbb{E} \left[\frac{\hat{p} + \tau(\mathbb{E}[\hat{p}] + \varepsilon)}{c_2} \right]. \quad (5)$$

Eq. (5) shows that the household equates marginal lifetime housing utility and expected housing-cost weighted marginal utility of future consumption. Increasing expected marginal tax payments through either higher τ or $\mathbb{E}[\hat{p}]$ therefore imply a greater willingness of the household to substitute housing with consumption. Approximating the integrals that correspond to the terms in expectation and assuming market clearing at fixed housing supply \bar{n} , we find the equilibrium per-unit transaction price given by

$$\hat{p}^o = \frac{W\theta}{(1 + \theta)\bar{n}} - \tau(\mathbb{E}[\hat{p}] + \delta q), \quad (6)$$

in which $\delta = (\beta + \theta + (1 + \beta)\theta)/(\beta(1 + \theta)) > 1$, as shown in more detail in Appendix B.1. The per-unit housing market clearing price, \hat{p}^o , thus depends negatively on the effective tax rate, τ , the per-unit expected AV, $\mathbb{E}[\hat{p}]$, as well as the assessment uncertainty, q . Hence, the higher and the more volatile future tax payments on a home, the lower the clearing price of the housing market.

To better see the effect of the tax channel, consider the post-publication market clearing price, in which the true per-unit AV, \hat{p} , is known by definition, and uncertainty, q , vanishes due to the two-period assumption⁸:

$$\hat{p}_{post}^o = \frac{W\theta}{(1 + \theta)\bar{n}} - \tau \hat{p}. \quad (7)$$

Eqs. (6) and (7) predict two dimensions through which taxation should influence transaction prices. First, a positive valuation shock, ε , should lead to a decrease in the market clearing price, \hat{p}^o , while a negative ε should lead to an increase. Second, greater uncertainty, q , should lead to a decrease in the market clearing price, \hat{p}^o , but only pre-publication.

⁸ Note that while the closed-form in Eq. (7) could be derived without integral approximations as all parameters are known, we continue using the result of Eq. (6) for the sake of consistency.

For both valuation shock and assessment uncertainty, the strength of implied price changes increases with the effective tax rate, τ .

The rational choice model described so far does not include anchoring. This is the case as anchoring itself is not rational. However popular advice given to potential buyers by real estate agents is to use an AV that is lower than the offer price as an argument for negotiating more effectively or to identify a home as potentially overvalued.⁹ Also, scientific research including Northcraft and Neale (1987) documents that even professional real estate agents adjust their appraisals towards randomly assigned ask prices. Similarly, Black and Diaz (1996) document random adjustments of ask prices to influence offering prices as well as final transaction prices in an experimental setting.¹⁰ A rational agent anticipating anchoring would simply adjust the optimal choice with respect to the purchase price to account for the heuristic bias. The anchoring-adjusted price function of a rational agent would then again coincide with the optimal decision, \hat{p}_{post}^o . In consequence, anchoring must result in a deviation from the optimal choice, unless the AV is a perfect predictor of the sales price before anchoring.

With anchoring, the final price per unit (and thus the overall price paid for a home) can be written as a linear combination of the per-unit price from Eq. (7) and the per-unit AV (see, e.g., Gibbs and Kulish, 2017). As anchoring can only take place after \hat{p} is known, i.e., after publication, the anchoring adjusted price, \hat{p}_{post}^a , is not affected by uncertainty, q , and thus given as

$$\hat{p}_{post}^a = (1 - \alpha) \left(\frac{W\theta}{(1 + \theta)\bar{n}} - \tau \hat{p} \right) + \alpha \hat{p}, \quad (8)$$

in which $0 \leq \alpha \leq 1$ is the equilibrium degree of anchoring. If $\alpha = 0$, \hat{p}^a corresponds to the market clearing price under rational choice, \hat{p}_{post}^o , and if $\alpha = 1$, the transaction price corresponds to the AV.

The comparison between the market clearing price before publication of \hat{p}^o and anchoring-adjusted post-publication price, \hat{p}_{post}^a reveals three equilibrium price effects through which AVs should affect transaction prices, two via unmatched expectations, ε , and one through uncertainty, q .

First consider the two effects that directly emerge through the valuation shock, ε , which are denoted anchoring and tax channel. The tax channel implies that ε is negatively related to the market clearing price, as a *ceteris paribus* higher valuation leads to increased tax payments, making a house-price unit less attractive to own. The anchoring channel, transferred through anchoring parameter, α , implies a counteracting effect. *Ceteris paribus*, a higher ε leads to an increased point of reference, which should increase the market clearing transaction price.

Technically, the effect of each channel on the transaction price can now be illustrated with the first derivative with respect to the valuation shock, ε , using $\hat{p} = \mathbb{E}[\hat{p}] + \varepsilon$,

$$\frac{\partial \hat{p}_{post}^a}{\partial \varepsilon} = -(1 - \alpha)\tau + \alpha, \quad (9)$$

which indicates that the tax channel prevails, i.e., an increasing AV decreases the transaction price, if $\alpha - (1 - \alpha)\tau < 0$, and the anchoring channel prevails, i.e., an increasing AV leads to an increasing sales price, if $\alpha - (1 - \alpha)\tau > 0$. Consequently, the two counteracting effects perfectly offset each other if $\alpha - (1 - \alpha)\tau = 0$.

The order of magnitude of each channel should be further influenced by (i) the drivers of the degree of anchoring, i.e., the determinants of α , and (ii) the individual effective tax rate τ . Accordingly, it

⁹ See, e.g., <https://real-estate-crunch.com/understanding-assessed-home-values-and-why-it-matters/#assessed-value> and <https://reiprime.com/assessed-value-vs-market-value-key-differences/> (both last accessed on March 11, 2025)

¹⁰ In the pioneering work of Tversky and Kahneman (1974), subjects are influenced in their judgment by a (seemingly) random wheel of fortune, demonstrating the irrationality of the anchoring heuristic.

should hold that first, the tax channel is most pronounced for units associated with high effective tax rates. Second, anchoring should be most prominent when AVs are most salient and up-to-date, i.e., right after publication. Both of these hypotheses are explored in Section 5.

The third price effect, transmitted through the tax-adjusted assessment volatility, τq , should affect house prices negatively before publication, but not afterwards. In reality, however, the overall price effect through publication of reassessments should be of less importance than the effects through the assessment shock, ε , as uncertainty persists due to the infinite time horizon after publication. Additionally, the assessment uncertainty effect should affect all homes in a given area homogeneously, limiting the implications for inequitable outcomes compared to the valuation shock channels. While our methodological approach therefore focuses on identifying the causal effect from both anchoring and tax channels, we additionally outline how we account and test for the uncertainty channel in the following section as well.

3. Methodology

In this section, we describe the design of our DD regression framework that allows for causal identification of the price effects described in the theory section. The first part of this section describes our approach to measure the impact of assessment shocks on property prices which are emanating from the anchoring channel and the tax channel. As described in the following Section 3.1, analyzing those effects in a regression model is challenging, as both imply heterogeneous price changes for over- and under-assessed homes, respectively, such that even within a treated jurisdiction, opposing price changes should be observed. After describing the transformations that solve this effect heterogeneity problem, we outline how we address the effect that stems from assessment uncertainty in Section 3.2. For the sake of clarity, we investigate each channel in isolation.

3.1. Measuring the impact of assessment shocks

As indicated above, homes with positive or negative valuation shocks face opposing price changes, even within the same jurisdiction, regardless of whether the anchoring or the tax channel prevails. Empirically, this effect heterogeneity needs to be addressed, as thus the aggregate effect on sales prices within a reassessing unit might well be zero, as individual assessment shocks are likely to cancel each other out. In consequence, regressions investigating changes in nominal prices are uninformative for the causal effect of assessment shocks on transaction prices.¹¹

We solve this issue by investigating the ratio between sales price and AV, $Dist(n)$, which is calculated by standardizing the absolute difference between the assessed value $AV(n)$ and the sales price $P(n)$ with the respective $AV(n)$, given as

$$Dist(n) = \frac{|P(n) - AV(n)|}{AV(n)} = \frac{|\hat{p}^a - \hat{p}|}{\hat{p}}. \tag{10}$$

It is important to standardize by $AV(n)$ rather than $P(n)$ in Eq. (10), as the AV remains constant before and after publication by definition, whereas, as illustrated above, the transaction price should be affected by anchoring and tax considerations. Consequently, dividing $Dist(n)$ by the transaction price, $P(n)$, instead of $AV(n)$ would yield an unstable and endogenous measure for the empirical application. As illustrated in the following, using $Dist(n)$ as dependent variable has the further advantage that the measurement of households' expectations is not required.

¹¹ Another approach would be to investigate subsamples of over- and under-assessed homes separately. Following this approach, however, does not help to draw definite causal conclusions, as the selective process involved prevents finding a suitable control group. Yet, as it is instructive to perform such an analysis as motivating and supporting evidence in event study form, we present such regressions in Section 5.3.

In the following sections, we show that $Dist(n)$ should change in opposing directions for the anchoring and tax channels. That is, relative to the control group, $Dist(n)$ should increase for all units in treated municipalities if the tax channel prevails, and decrease for all units in treated municipalities if the anchoring channel prevails. A possible challenge with our $Dist(n)$ measure may arise if $AV(n)$ is not equal to $\mathbb{E}[P(n)]$. If, for example, assessed values are systematically lower than observed trade prices, such as under Swiss tax law, where assessed values for tax purposes are deliberately 20 to 40% below trade prices to reduce the property and wealth tax burden on homeowners,¹² then the $Dist(n)$ -measure does not necessarily increase for homes that are overvalued relative to similar other homes if the tax channel prevails. To account for possible systematic under- or overassessment, we standardize $P(n) - AV(n)$ in the numerator of our $Dist(n)$ -measure in Eq. (10) to have mean zero for every tax year and municipality, thereby also accounting for possible differences in assessments over time and through municipalities. In the following, we also refer to our normalized $Dist(n)$ -measure as the $Dist(n)$ -measure for simplicity. For the sake of clarity, the following two sections separately investigate each channel's effect on our normalized $Dist(n)$.

3.1.1. Anchoring channel

In this section, we show that the anchoring channel should reduce the relative distance between sales price and AV, $Dist(n)$. Intuitively, this is the case as anchoring moves the transaction price towards the AV, reducing the distance between both quantities, $Dist(n)$.

Formally, define $\hat{p}^* := W\theta/(\bar{n}(1+\theta))$. Then, isolating the anchoring channel, the change in the relative sales-price-AV difference from pre to post AV publication can be described with

$$Dist(n)^{pre} - Dist(n)^{post} = \frac{|\hat{p}^* - \hat{p}|}{\hat{p}} - \frac{|(1-\alpha)\hat{p}^* + \alpha\hat{p} - \hat{p}|}{\hat{p}} = \alpha \frac{|\hat{p}^* - \hat{p}|}{\hat{p}}. \tag{11}$$

Note that anchoring is not possible pre-publication since $\hat{p} > 0$ is not yet known. From Eq. (11), it holds that $Dist(n)^{pre} \geq Dist(n)^{post}$, as $\alpha \geq 0$. Notably, anchoring strictly reduces $Dist(n)$ for all homes, whether over- or under-assessed, given that \hat{p} does not predict \hat{p}^* perfectly.

3.1.2. Tax channel

In contrast to the anchoring channel, the tax channel generally implies an opposite, increasing effect on $Dist(n)$. Intuitively, an unexpectedly higher AV typically leads to a lower sales price, such that the distance between both quantities, $Dist(n)$, increases. In the opposite case, an unexpectedly lower AV increases the transaction price, again increasing $Dist(n)$. The formal conditions under which this intuition holds are derived in this section.

As shown in Eq. (6), households form expectations about their future tax burden, $n\tau\mathbb{E}[\hat{\rho}]$. Hence, before updated AVs are published, households purchase their home under the expected future AV as in Eq. (6), while making use of the true AV, $\hat{\rho}$, once AVs are updated, as shown in Eq. (7). Thus, when considering the tax channel isolated from anchoring, it holds that

$$Dist(n)^{pre} - Dist(n)^{post} = \frac{|\hat{p}^* - \tau\mathbb{E}[\hat{\rho}] - \hat{p}|}{\hat{p}} - \frac{|\hat{p}^* - \tau\hat{\rho} - \hat{p}|}{\hat{p}}. \tag{12}$$

In Appendix B.2, we show that the conditions under which $Dist(n)^{pre} \leq Dist(n)^{post}$ holds, i.e., that the tax channel has an increasing effect on $Dist(n)$, depend on the size of the true AV relative to household expectations, $\mathbb{E}[\hat{\rho}]$. The most likely case in which $Dist(n)$ increases due to the tax channel is when the household's expectations about the AV, $\mathbb{E}[\hat{\rho}]$, lie between the actual AV, $\hat{\rho}$, and the pre-publication price. Then, the tax channel has an increasing effect on $Dist(n)$, regardless of whether a home is over- or under-assessed. Most importantly, as $Dist(n)$, as well as the post-publication change in $Dist(n)$, are observable for both treated and control group, respectively, the methodological approach explained in the following section does not need to rely on the measurement of household expectations.

¹² A similar effect could also be achieved with lower property tax rates, but the Swiss tax law has chosen a different way.

3.2. Measuring the impact of assessment uncertainty

A third equilibrium price effect emerges through aversion to assessment risk. That is, the more volatile the valuation shock, q , and the higher the effective tax rate, τ , the more reluctant households are to buy an additional unit of housing. That is, because the possibility of paying excessive taxes due to a large and positive valuation shock outweighs the potential utility gains from a negative shock of the same magnitude. Empirically, we address the uncertainty channel in two ways. First, q is measurable on the local level, as the distribution of AV changes can be recovered from the data, as well as the local effective tax rate, τ . Hence, it is possible to construct the tax-adjusted assessment volatility, τq , as control variable for the DD regressions. Additionally, in Section 5.3, we provide direct evidence in sales price regressions.

In the DD regressions using the empirical counterpart of $Dist(n)$ as dependent variable, the effect of the tax-adjusted volatility, τq , should be observed as follows. First, as uncertainty about future valuation should negatively affect transaction prices, $Dist(n)$ should be decreased if a home is under-assessed, i.e., when the AV is lower than the corresponding transaction price. Specifically, increased tax-adjusted volatility decreases the transaction price, which drives it towards the (lower) AV. In contrast, the effect of the tax-adjusted volatility on $Dist(n)$ should be positive for over-assessed homes, as a higher tax-adjusted volatility leads to a sales price that is even lower, driving the transaction price further away from its AV. In the following, we present our regression model that allows to analyze the effects described above.

3.3. Regression model

The aim of this paper is to analyze the feedback effect of AVs on transaction prices. To be able to interpret the results causally, we run DD regressions that compare pre- and post-publication transactions of reassessed and non-reassessed homes. Whether homes are reassessed in a given tax year depends on the municipality's own reassessment schedule. As our dataset contains multiple years of data for a large number of jurisdictions, we define municipality-tax-year clusters c by employing symmetric time-windows of ± 180 days around each publication deadline T_c . We do not consider longer time-spans to avoid overlaps between treatment and control clusters.

We define a dummy variable $Treat_{itc}$, which equals one if home i is sold within a municipality-tax-year cluster $c(t)$ in which homes have been reassessed collectively, i.e., an updated AV is available for all respective homes, and zero otherwise.¹³ We follow the standard DD framework by defining a dummy variable $Post_{itc}$ that equals one if home i was sold after T_c , and zero otherwise. Consequently, we run regressions of the form

$$Dist_{itc} = \beta_0 + \beta_1 Treat_{itc} + \beta_2 Post_{itc} + \gamma Treat_{itc} \times Post_{itc} + \beta_3 X_{itc} + \kappa_{ic} + \nu_t + \eta_{itc}, \quad (13)$$

in which $Dist_{itc}$, as defined in Eq. (10), is the absolute ratio between sales price of home i , P_i , and its AV in the corresponding tax-year cluster c , β_0 is an intercept, κ_{ic} and ν_t denote location-tax-year (e.g., municipality-tax-year) and temporal (tax-year-quarter) fixed effects, respectively, $Treat_{itc} \times Post_{itc}$ is the interaction between the dummies identifying treatment and post groups, respectively, X_{itc} is a set of control variables, and η_{itc} is a nuisance term.

The control variables contained in X_{itc} include prior year's log-AV, to account for different initial price levels among homes, and a tax-adjusted assessment volatility measure to account for the uncertainty effect τq predicted in the theoretical model in Section 2. To account for the prediction that uncertainty plays a greater role before publication than afterwards, the variable is interacted with the "Post" dummy.

We standardize all control variables to have mean zero and unit standard deviation to simplify interpretation. As tax-adjusted assessment volatility is zero for the control group by definition, we standardize this particular variable using the subsample of treated units only.

The coefficient of interest, γ , measures the treatment effect on $Dist_{itc}$ from the assessment shock, ε . As illustrated in Section 3.1, if γ is positive, the tax channel prevails. In contrast, the anchoring channel outweighs the tax channel if γ is negative. Before presenting the empirical results, we introduce the data used and discuss the validity of the identification strategy in the following section.

4. Data

In this section, we present summary statistics for the variables used in our work. A detailed description of the data cleaning and the construction of our variables can be found in Appendix C.

Table 1 provides summary statistics of key variables used in the later analyses. Panel A reports statistics for the treated units, Panel B for units sold within zero-change control clusters, and Panel C for units from constant-adjustment control clusters. From the total 355,951 observations, about 43% are treated units. For them, the average sales price is about 393,000 USD, while the mean AV is slightly lower at about 385,000 USD. In contrast, the control groups contain units that tend to be comparably lower priced with an average sales price of 259,000 USD in the zero-change group and 306,000 USD in the constant-adjustment group. For the treatment group, the average AV increase is 0.8%, reflecting the general increase in house prices over the sample period, but also that the sample includes the post-bubble period from 2006 to 2010. With a standard deviation of more than 9% in the treated group, substantial changes in the AV from one year to another appear to be rather common, which is also reflected in the average local assessment volatility of 7%. The local effective tax rate is higher on average for both control groups. One reason for this observation could be the need to increase tax rates in lower-priced areas to generate sufficient tax revenue. The difference between control groups is illustrated by the one-year AV growth variable, which is zero for the zero-adjustment group by definition, and the local assessment volatility, which is zero for both groups.

Fig. 1 illustrates the geographic distribution of transactions within the state of New York, separated by treatment status. Panel A shows transactions for treated units, and Panel B for control ones. Unsurprisingly, properties in New York City, found in the southeast of the state, are all treated units, as here, homes are reassessed annually. Other cities, such as Buffalo, located in the western part of the state, reassess less frequently, so homes from Buffalo can be found in both groups. The comparison of Panels A and B illustrates why the control group should match the treated units well. There are many overlaps among cities and towns, such that the AVs of both groups should be similarly precise and distributed, as the local assessors should remain relatively stable over time.

5. Results

The aim of this work is to show the impact of AVs on sales prices. To causally identify this effect in a DD framework, we investigate $Dist_{itc}$ and exploit the timing of publication at time T_c in a time window of ± 180 days.¹⁴ The sign of this effect is not clear ex ante. As illustrated in

¹⁴ We acknowledge that directly exploring how changes in assessed values affect changes in observed prices would be more straightforward to interpret. Unfortunately, changes in prices are only observable on a very infrequent basis, as they require two trades of the same home (as opposed to only one trade under our approach), rendering this approach unappealing to apply for homes. We nevertheless report the results of regressing changes in observed prices on changes in AVs using the substantially smaller sample of repeat-sales in Appendix E. Overall, the results from this analysis support those of our causal Diff-in-Diff regressions: Homes with larger AV increases face lower price increases, supporting the tax channel.

¹³ Instead of $Treat_{i,c(t)}$ we write $Treat_{itc}$ for ease of notation.

Table 1
Summary statistics.

Panel A: Treated group						
	Observations	Mean	Std	1st Quantile	Median	3rd Quantile
Sales price	152,783	392,520	446,375	150,000	315,000	479,000
Assessed value (AV)	152,783	384,689	402,008	148,400	322,000	474,200
Abs. price-AV ratio (Dist)	152,783	0.156	0.162	0.045	0.105	0.212
Abs. price-AV diff.	152,783	64,079	135,377	10,000	29,300	71,000
One-year AV growth rate	152,783	0.008	0.094	-0.042	0.000	0.041
Local effective tax rate	152,783	0.020	0.011	0.009	0.020	0.029
Local assessment volatility	152,783	0.072	0.033	0.054	0.070	0.096
Post	152,783	0.500	0.500	0.000	1.000	1.000
Crisis (2006–2010)	152,783	0.357	0.479	0.000	0.000	1.000
Panel B: Zero-change control group						
	Observations	Mean	Std	1st Quantile	Median	3rd Quantile
Sales price	102,613	259,023	322,674	112,200	180,000	316,000
Assessed value (AV)	102,613	250,104	289,735	109,600	180,200	310,985
Abs. price-AV ratio (Dist)	102,613	0.163	0.166	0.049	0.112	0.220
Abs. price-AV diff.	102,613	44,271	104,957	8,370	20,300	46,800
One-year AV growth rate	102,613	0.000	0.000	0.000	0.000	0.000
Local effective tax rate	102,613	0.028	0.008	0.024	0.029	0.034
Local assessment volatility	102,613	0.000	0.000	0.000	0.000	0.000
Post	102,613	0.552	0.497	0.000	1.000	1.000
Crisis (2006–2010)	102,613	0.227	0.419	0.000	0.000	0.000
Panel C: Constant-adjustment control group						
	Observations	Mean	Std	1st Quantile	Median	3rd Quantile
Sales price	100,555	306,136	329,717	142,500	245,000	375,000
Assessed value (AV)	100,555	291,930	295,669	138,889	242,717	355,814
Abs. price-AV ratio (Dist)	100,555	0.189	0.182	0.061	0.136	0.258
Abs. price-AV diff.	100,555	58,763	113,902	13,500	32,200	68,158
One-year AV growth rate	100,555	0.007	0.065	-0.040	0.010	0.044
Local effective tax rate	100,555	0.027	0.008	0.021	0.027	0.031
Local assessment volatility	100,555	0.000	0.000	0.000	0.000	0.000
Post	100,555	0.520	0.500	0.000	1.000	1.000
Crisis (2006–2010)	100,555	0.420	0.494	0.000	0.000	1.000

This table provides summary statistics for the variables used in the empirical application. Panel A shows summary statistics for the homes located in treated municipality-tax-year-clusters c , i.e., municipalities in which homes have been reassessed in a given tax year surrounding the respective AV-publication date. Panel B shows the same statistics for zero-change control units, i.e., sales of homes within municipality-tax-year clusters that left AVs unchanged in a given tax year. Panel C describes units in the constant-adjustment control group, i.e., units sold in municipality-tax-year-clusters in which homes have not been reassessed, but AVs were collectively adjusted by the same constant. “Sales price” denotes the nominal transaction price, and the “Assessed value (AV)” is the publicly available estimated market value provided by the local assessor, which is unknown if the corresponding unit was sold before the publication date T_c , and known afterwards. “Abs. price-AV ratio (Dist)” is the normalized $Dist_{itc}$ -measure introduced in Section 3.1. “Abs. price-AV diff.” is the absolute value of the difference between nominal sales price and AV. “One-year AV growth rate” is the relative change in AV with respect to the prior year. The “Local effective tax rate” is a municipality’s median effective tax rate (tax amount paid divided by the respective AV) in a given tax year. If information about tax payments is missing for a given year, the one-year lag of the median tax rate is used. “Local assessment volatility” is the standard deviation of the one-year AV growth rate on the municipality-tax-year level. “Post” is a dummy indicating whether a property was sold before or after the publication of AVs. “Crisis (2006–2010)” is a dummy that equals one if the property value was published between 2006 and 2010, and zero otherwise.

Section 2, the tax channel should lead to an increase in $Dist_{itc}$ through an induced change in tax payments. In contrast, anchoring should reduce $Dist_{itc}$. Using municipality-tax-year clusters where homes are not reassessed as a control group, we run DD regressions following Equation (13). To further analyze the impact of valuation uncertainty, predicted by Eq. (6), we control for the local tax-adjusted assessment uncertainty before and after publication within the regression analysis. In the following section, we first provide evidence for the validity of our DD regressions in form of pre-publication differences between treatment and control groups. Subsequently, we outline our baseline results and discuss their economic implications.

5.1. Common trends and baseline results

The key underlying assumption of the DD analysis is that the trend of the dependent variable $Dist_{itc}$ would have been the same for the treated and control groups in the absence of treatment, i.e., that of a common trend. If this assumption holds, the control units can be plausibly used to infer the counterfactual outcome of the treated units. In other words, the homes in the control group inform about how the dependent variable $Dist_{itc}$ of the treated homes would have developed without the current reassessment.

Panels A to D of Fig. 2 present both supportive evidence for the common trend assumption and the baseline results. Each panel, based on either the full sample or different subsamples, depicts monthly treatment effects in equidistant intervals of 30 days surrounding the publication date, which is the first day of month “0”. The final pre-publication month is highlighted by the vertical line at month “-1”. Each panel depicts point estimates from a single respective regression that includes the baseline control variables and fixed effects (i.e., zip-code-tax-year and tax-year-quarter fixed effects). Base-month is month “-2” and confidence bounds, based on county-level clustered standard errors, indicate the 95% significance level.¹⁵

All Panels of Fig. 2 show similar key features. First, pre-publication, there is no significant difference between treatment and control group, almost all point estimates are insignificant at the 5% level. Second, beginning with the first post-publication month, “0”, point estimates increase, in line with the tax channel. Intuitively, a higher assessed

¹⁵ An inspection shows that the base case results are robust to alternative clustering of standard errors, such as on municipality or zip-code level, as well as (Cameron et al., 2011) two-way clustering over counties and tax years.

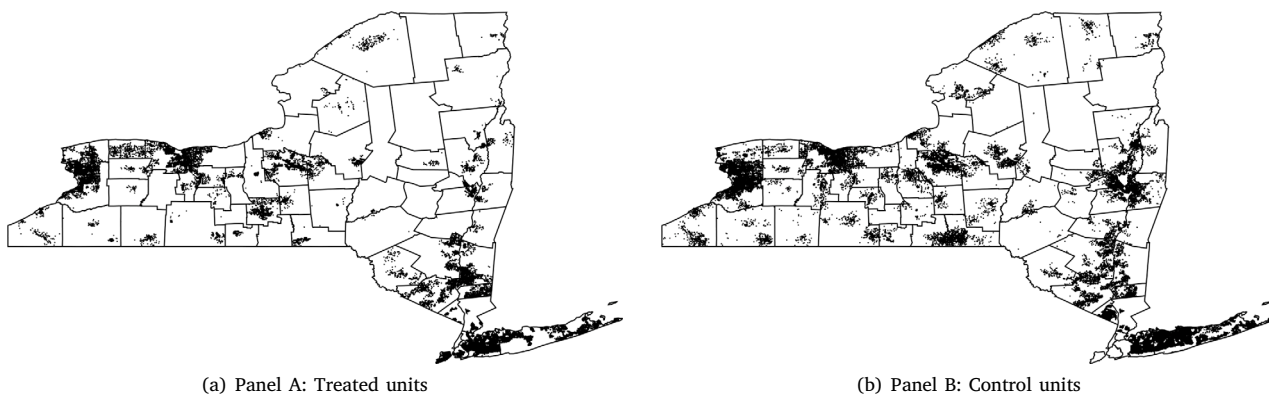


Fig. 1. Geographic dispersion of transactions

Panels A and B of this figure show the geographic dispersion of observations in the cleaned dataset within New York State. Note that observations from smaller areas are excluded during the data cleaning process, in order to increase comparability between the treatment and control group. Panel A shows the distribution of observations assigned to the treatment group, i.e., sales that took place in a time window of ± 180 days around the publication of preliminary reassessments. Panel B shows the geographic distribution of homes in the control group, i.e., those that were transacted ± 180 days around the AV publication deadlines in tax-years in which homes have not been reassessed (i.e., AVs either coincide with last year's AV or AVs are collectively adjusted by the same constant). The solid lines indicate county and state borders. County and state border data is provided by the US Census Bureau.

value leads to a lower transaction price, which increases the dependent variable $Dist_{itc}$. Third, after an adjustment period, the treatment effect stabilizes on a new level. This could indicate that the anchoring channel is present right after publication, when AVs are most salient and most up-to-date. Over time, AVs get more and more outdated, leading to increasing dominance of the tax channel until the new equilibrium is reached.

Panel A of Fig. 2, displaying the results for the full sample, is characterized by a striking feature that needs to be discussed. Specifically, an increase in the dependent variable from month “-2” to “-1” is observed. While this would seem like a violation of the common trend assumption, as no such increase should be observed before the publication date, it is not contradictory to the setup of our empirical strategy. Importantly, our publication date is not necessarily fixed, but constitutes a deadline for publication. Hence, the pre-event increase could merely reflect that some of the local assessors send out notices days before the deadline. When investigating more homogeneous samples, such as using the more standardized sample when including municipalities with publication month March only (Panel B), excluding New York City (NYC, Panel C), or looking at the sub-period of recovery from 2011–2017 (Panel D), this pre-trend vanishes almost entirely. In particular, Panel B further indicates that municipalities that follow the official New York State tax calendar (hence publishing in March) are more likely to publish updated AVs more tightly to the schedule. In contrast, if the slight upwards trend before publication in Panel A existed merely due to a random development as the confidence bounds suggest, it would work against the effect that we document.

Having outlined our baseline results graphically, we now turn to provide estimates corresponding to the full post-publication period for varying model specifications. Table 2 shows the results for OLS regressions with $Dist_{itc}$, the normalized absolute sales-price-AV ratio, as the dependent variable. Panel A shows the results for the treatment effect due to assessment shock, which is given by the interaction between the dummy variables “Treatment” and “Post”. Panel B yields results for the control variables related to the tax-adjusted assessment volatility, and Panel C shows coefficient estimates for all other controls included in the analyses. Standard errors are clustered over counties and are shown in parentheses below. Column (1) shows estimates when only municipality-tax-year fixed effects are included. As indicated in Panel A, the treatment effect from the assessment shock is estimated at 1.3%. The coefficient is statistically significant at the 0.1% level and indicates that knowledge about new AVs influences $Dist_{itc}$ positively, in line

with the tax channel. The point estimate confirms the results from Fig. 2. An increased assessed value decreases the transaction price of the respective home.

Column (2) includes additionally tax-year-quarter fixed effects, in which each tax year is split into quarters relative to the event date, thus additionally accounting for temporal variation across districts. The result remains significant at the 0.1% level and suggests that AVs have a positive causal effect on $Dist_{itc}$ of 1.4 percentage points. Using the more fine-grained zip-code-tax-year fixed effects as shown in column (3) does not alter the treatment effect up to the third digit, indicating that the location-based fixed effects are sufficiently disaggregated. For columns (4) and (5), additional control variables are included. Most importantly, following the prediction of our model in Eq. (6), we control for the tax-adjusted assessment volatility. The results for this variable are discussed later below. Together, the results for both models document an overall significant treatment effect of 1.1 percentage points that again confirm a prevailing tax channel.

To provide an economic interpretation of this causal effect, we conduct a back-of-the-envelope calculation making use of the relationship

$$\Delta P_{ic} = \frac{\gamma AV_{ic}}{P_{ic}^c}, \tag{14}$$

in which P_{ic}^c is the price under control conditions, and ΔP_{ic} the relative change in the sales price between treatment and control state. The coefficient γ is the treatment effect, which is estimated at 1.1% in the base case. The simplifying assumption that allows this closed-form solution is that both transaction prices, under both treated and control conditions, are larger than the AV, as shown in Appendix B.3.¹⁶

Applying Equation (14) to the sample means of the treatment group, it is possible to derive an effect on transaction prices. Plugging in the average AV of the treatment group (about 393,000 USD), and using the average difference between the sales price and the AV of 64,000 USD (which must be subtracted from the sales price such that the underlying assumption is fulfilled), the relative change in P_{ic} due to treatment is about one percent. This indicates that value-based property taxation

¹⁶ This assumption is in line with the sufficient condition (B.30), which implies an increase in the sales price post-publication as well. Doing the same calculations, but assuming instead that the transaction prices are smaller than the AV, a decline in the transaction price is implied. Again, this decline is in line with the corresponding condition (B.32).

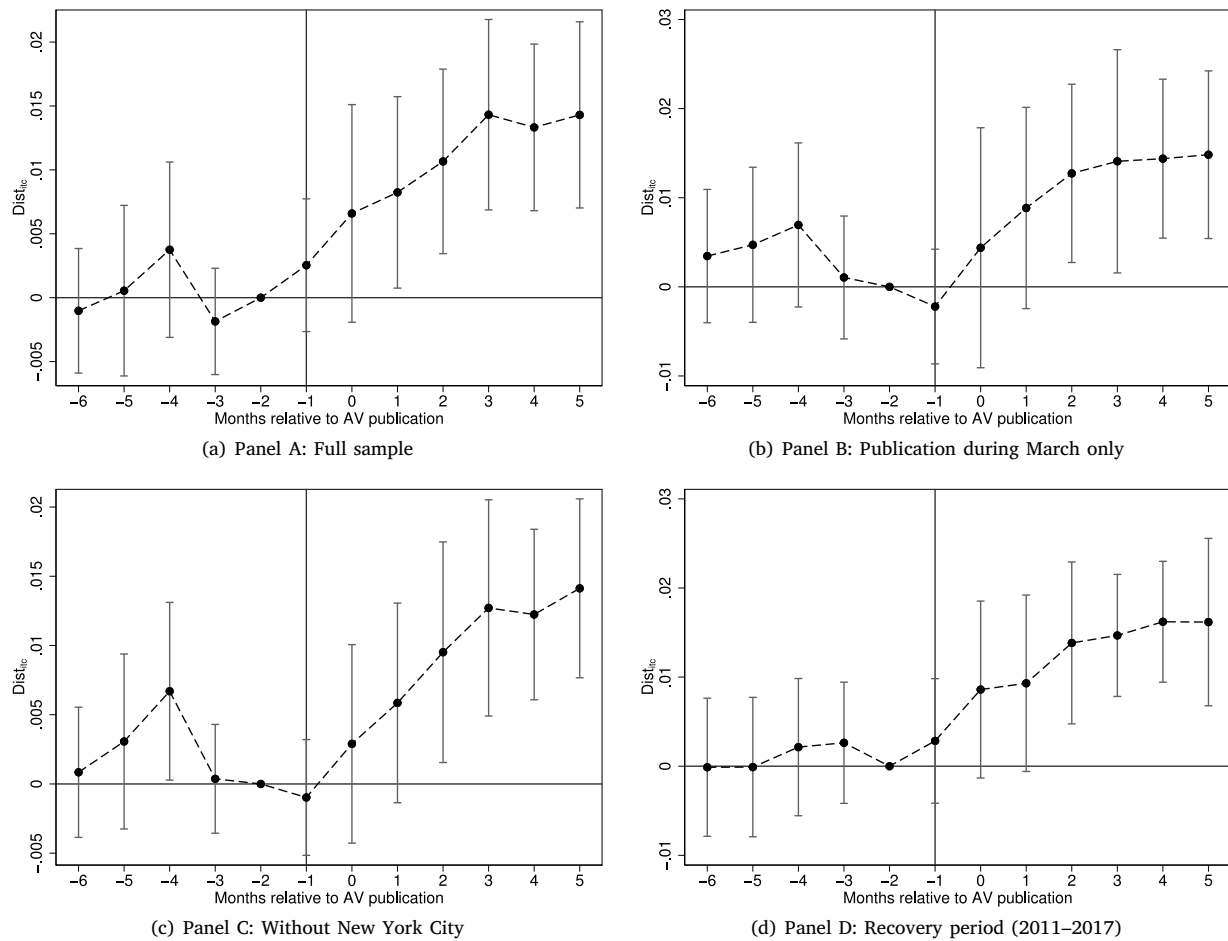


Fig. 2. Monthly treatment effects relative to local publication deadline

Panels A-D of this figure depict monthly treatment effects relative to the publication date at the beginning of interval zero (“0”) for the respective sample indicated in the sub-headline. Specifically, the point estimates depict the evolution of $Dist_{itc}$ as the normalized $Dist$ -measure introduced in Section 3.1 of homes transacted during tax years of reassessment (i.e., treated units) relative to homes transacted during tax years without reassessment (i.e., control units). Control units are located in either zero-change or constant-adjustment municipalities. Months are calculated in equidistant intervals of 30 days relative to a given municipality’s publication date. For instance, the interval labeled “0” includes the publication deadline as well as the 29 days thereafter. Each panel depicts regression coefficients of a single regression, each of which includes zip-code-tax-year and tax-year-quarter fixed effect dummies, as well as all base case control variables. Base category is month “-2”. The regression displayed in Panel A analyzes the full sample. For the results in Panel B, only observations with the most common publication calendar month (March) are selected. For Panel C, homes from New York City (NYC) are excluded. Panel D uses transactions made during tax years 2011–2017 only. The vertical lines around the point estimates indicate 95% confidence bounds, which are based on standard errors clustered on the county-level.

leads to economically significant price distortions of about one percent of the counterfactual sale price.

Panel B of Table 2 shows results for the assessment uncertainty control variables. In line with the intuition provided by the theoretical model in Section 2, only the pre-publication variable significantly predicts changes in the dependent variable. Once AVs are published, there is less (or no) assessment uncertainty remaining, resulting in an insignificant coefficient estimate for the “Post” interaction variable. Given that about 56% of the treated units are under-assessed (i.e., have an AV that is lower than the sales price), the negative sign of the coefficient estimate is also in line with the model. Specifically, a larger tax-adjusted assessment volatility decreases the transaction price, which increases the sales-price-AV distance. Consequently, an opposite sign is expected for over-assessed homes (the remaining 44% of treated observations). We thus split the uncertainty regressors further by using a dummy indicating over-assessment. The positive sign for the “Pre”-publication effect is again in line with our hypotheses, as well as the estimate close to zero corresponding to the “Post”-publication estimates.

Panel C of Table 2 shows the results for the remaining model controls. We include past tax-year’s AV as control in order to capture that

the assessment precision, measured by $Dist_{itc}$, might vary over house price segments. The negative coefficient for the AV lag indicates a more precise assessment for pricier homes, conditional on control variables. This finding is in line with the results of Berry (2021), who finds that lower-priced homes tend to be over-assessed, i.e., less precisely assessed. As column (5) interacts the tax-adjusted assessment volatility regressors with an “Over-assessed” dummy, we further include controls related to over-assessment to rule out that systematic differences of such homes are driving our results.

The results presented so far indicate that the tax channel prevails. If this is indeed the case, transactions that involve units associated with higher effective tax rates should exhibit stronger effects, as illustrated in Eq. (9). To test this prediction, we analyze subsamples selected on the basis of the median of the local effective tax rate in a given tax year. We run separate DD regressions for both subsamples using the same controls as in the base case. The underlying common trend assumption for both subsamples is supported by pre-publication trends, as further discussed in Section Appendix F of the Online Appendix.

Table 3 shows the results when separated by effective tax rates. The estimated treatment effects are about 1.1 percentage points for the

Table 2
Base case results.

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Assessment shock</i>					
Treatment effect	0.013*** (0.002)	0.014*** (0.003)	0.013*** (0.002)	0.013*** (0.002)	0.011*** (0.003)
<i>Panel B: Assessment uncertainty controls</i>					
Pre × Tax-adj. vola				-0.015*** (0.004)	-0.023*** (0.004)
Post × Tax-adj. vola				-0.007* (0.003)	-0.009* (0.004)
Over-assessed × Pre × Tax-adj. vola					0.021** (0.007)
Over-assessed × Post × Tax-adj. vola					0.004 (0.005)
<i>Panel C: Other controls</i>					
ln(AV _{<i>i,t-1</i>})				-0.070*** (0.006)	-0.073*** (0.006)
Over-assessed					0.043*** (0.006)
Over-assessed × Treatment					-0.008 (0.006)
Location-tax-year fixed effects	mun.	mun.	zip-code	zip-code	zip-code
Tax-year-quarter fixed effects	No	Yes	Yes	Yes	Yes
Adj. R-sq.	0.093	0.093	0.094	0.134	0.147
Observations	355,951	355,951	355,951	355,951	355,951

This table provides coefficient estimates on five separate difference-in-differences regressions based on Equation (13), with $Dist_{itc}$ as the normalized $Dist$ -measure introduced in Section 3.1 as the dependent variable. The “Treatment effect” is the coefficient estimate of the interaction between the “Treatment” and “Post” dummies. The “Treatment” dummy equals one if the unit was sold within a municipality-tax-year cluster in which homes have been reassessed and zero otherwise. The “Post” (“Pre”) dummy equals one (zero) if the unit was sold after AV publication, and zero (one) otherwise. “Tax adj. vola” is the local effective tax rate times the local assessment volatility. “Over-assessed” is a dummy indicating that the AV of a home is higher than its sales price. “AV_{*i,t-1*}” is the assessed value of the immediately preceding tax year. “Location-tax-year fixed effects” indicate fixed effect dummies in a given area (municipality or zip-code level), interacted with a given tax-year dummy. All continuous control variables are standardized to zero mean and unit standard deviation. “Tax-adj. vola” is standardized based on the treated subsample only. “×” indicates an interaction between two variables. Standard errors are clustered over counties. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively.

Table 3
High vs. low effective tax rates.

	High local tax	Low local tax
Treatment effect	0.012*** (0.002)	0.011*** (0.003)
Adj. R-sq.	0.156	0.147
Observations	177,676	178,275
Control variables	Yes	Yes
Zip-code-tax-year fixed effects	Yes	Yes
Tax-year-quarter fixed effects	Yes	Yes

This table provides coefficient estimates on two separate difference-in-differences regressions based on Equation (13), with $Dist_{itc}$ as the normalized $Dist$ -measure introduced in Section 3.1 as the dependent variable. To run both regressions, the sample is divided according to the median of the local effective tax rate. The “High local tax” (“Low local tax”) subsample is based on observations with local effective tax rates of above (below or equal to) the sample median. The “Treatment effect” is the coefficient estimate of the interaction between “Treatment” and “Post” dummies. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. The control group includes both, the zero-change and the constant-adjustment group. “Post” is a dummy that equals one if the unit was sold after AV publication, and zero otherwise. “Control variables” are all AV, local tax-adjusted volatility, and over-assessment related controls as in column (5) of Table 2. Again, all control variables are standardized to have zero mean and a standard deviation of one. Standard errors are clustered over counties. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively.

low-tax sample and 1.2 percentage points for the high-tax subsample. Together, the findings on the tax subsamples thus provide further

suggestive evidence that the tax channel might be more important for the AV effect on sales prices.

5.2. Disentangling the treatment effect

Our results from Table 2 reveal that the tax-channel dominates the anchoring channel. However, our treatment effect only measures the aggregate effect. It remains silent about whether the aggregate effect is driven by a very strong tax effect accompanied by a weaker anchoring effect or whether the treatment effect essentially covers the tax channel with the anchoring effect being unimportant. In this section, we provide empirical evidence in support of the latter explanation. To do so, we conduct two independent investigations.

In the first investigation, we quantify the present values of the average reassessment-induced additional tax burden and compare it to our model-implied price effect. We focus on homes that were both sold and reassessed with a higher assessment in a given year. First, we compute the relative change in the property tax burden between two consecutive trades. Second, we calculate the absolute dollar-valued perpetuity-implied present-value of the additional tax burden assuming a 4% interest rate, roughly corresponding to the average 15-year mortgage rate in our sample period according to FRED data. Third, we calculate the ratio of this value and the more recent trade price to get the relative present-value effect. If we see an effect in the order of magnitude of about 1%, the average price effect from Eq. (14), we could conclude that the anchoring effect does not seem to play a major

Table 4
High vs. low supply elasticities.

	High supply elasticity	Low supply elasticity
Treatment effect	0.013*** (0.002)	0.011* (0.004)
Adj. R-sq.	0.148	0.137
Observations	149,881	149,871
Control variables	Yes	Yes
Zip-code-tax-year fixed effects	Yes	Yes
Tax-year-quarter fixed effects	Yes	Yes

This table provides coefficient estimates of two separate difference-in-differences regressions based on Equation (13), using $Dist_{itc}$ the normalized $Dist$ -measure introduced in Section 3.1 as the dependent variable. To run both regressions, the sample is divided according to the median of the Baum-Snow and Han (2023) sensitivity variable $gamma01b_units_FMM$ based on the census tracts. This variable has been merged with the baseline dataset using shapefiles containing geospatial information that have been downloaded from www.census.gov. The “High supply elasticity” (“Low supply elasticity”) subsample is based on observations with elasticities that are above (below or equal) the sample median. The “Treatment effect” is the coefficient estimate of the interaction between “Treatment” and “Post” dummies. “Treatment” is a dummy that equals one if the unit was sold within a municipality-tax-year cluster in which AVs were updated, and zero otherwise. The control group includes both, the zero-change and the constant-adjustment group. “Post” is a dummy that equals one if the unit was sold after AV publication, and zero otherwise. “Control variables” are all AV, local tax-adjusted volatility, and over-assessment related controls as in column (5) of Table 2. Again, all control variables are standardized to have zero mean and a standard deviation of one. Standard errors are clustered over counties. **,*, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively.

role. Our empirical average reassessment-induced additional tax burden is 3.3%. That is, we observe a substantially larger effect, which could suggest that anchoring plays a role. The larger effect could, however, also indicate that institutional constraints, such as caps on how fast property tax burdens can increase, limit the insights from our analysis.

In order to nevertheless provide more conclusive evidence, we employ a second investigation. This investigation builds on the intuition that if the anchoring effect exists and is at least reasonably strong, an unexpected increase in the AV should increase the trade price in areas with elastic supply of housing, as in such areas the anchoring effect could possibly dominate the tax effect. That is, in areas with elastic supply of housing, the $Dist(n)$ -measure should decrease if there exists a reasonably strong anchoring effect. To investigate this question empirically, we exploit differences in housing supply elasticities on the micro level published by Baum-Snow and Han (2023). More precisely, we split our sample at the median into areas with high and low supply elasticity using the variable $gamma01b_units_FMM$ published on the authors’ homepages and merge this data with the *CoreLogic* data using shapefiles with geospatial data from www.census.gov and estimate the strength of the treatment effect separately for the high and the low elasticity subsample.¹⁷

Our results from Table 4 show that the treatment effect is of similar magnitude across the high and low elasticity subsamples. In both the high and the low supply elasticity case, the treatment effect is positive. The point estimate of the treatment effect is even slightly higher in areas with higher supply elasticity, which is at odds with standard economic theory and could suggest an overreaction to the tax effect these areas.¹⁸ However, the two coefficient estimates are not significantly different from each other, which clearly speaks against

¹⁷ We also performed a split into terciles comparing the treatment effect of the highest with the lowest tercile, leading to the same conclusion as the results outlined here. Due to missing geospatial data in the *CoreLogic* data set for some observations, our sample size is slightly smaller than in our base case. We thank Nathaniel Baum-Snow for sharing additional insights on the housing supply elasticity data that helped us merging it to the *CoreLogic* data.

¹⁸ The difference may also reflect that housing supply reacts asymmetrically. Even though a market may be elastic with respect to price growth, it might be less so with respect to negative price adjustments (Glaeser and Gyourko, 2005).

the existence of a strong anchoring effect and instead indicates that the tax effect dominates. Overall, our results from this section strongly suggest that, empirically, anchoring does not play an important role – if any – and instead supports the argument that the treatment effect is essentially a pure tax effect.

5.3. Supporting evidence: An event study

Although based on a theoretical framework and allowing for causal inference, the quasi-experimental analyses presented in this paper so far do not provide directly interpretable evidence due to the necessary transformations, for instance, when calculating the dependent variable $Dist_{itc}$. Hence, to provide more intuitive and simplified evidence, we run several event study regressions making use of the treated units only (thus departing from a control group). Here, we provide a pre- and post-publication comparison of how assessment shocks, estimated in a first-stage regression, are related to sales prices. Intuitively, we thus analyze whether there is an excess effect of post-publication assessment shocks on sales prices compared to the naturally existing (full-sample) relationship. Additionally, we compare the predictive power of tax-adjusted assessment volatility before and after publication.

Within a first-stage regression, we derive assessment shocks as the abnormal AV growth rate of a home relative to other properties in the same zip-code in a given tax year. In this regression, we additionally include each home’s lagged (log) AV to account for heterogeneity in appreciation rates across price segments. We allow this relationship to vary over jurisdictions by interacting the AV regressor with municipality-level dummies. We provide further technical details, as well as regression equations in Appendix D.

Under the tax channel, a higher assessment shock should lead to a decrease in the transaction price post-publication, while under the anchoring channel, the same shock should increase the value of the underlying home, once the true AV growth rate is known. Hence, a negative sign for the assessment shock supports the previous finding that the tax channel prevails, while a positive sign would yield contradictory evidence. Panel A of Table 5 shows coefficient estimates for the assessment shocks for different model specifications. For the baseline specification displayed in column (3), the coefficient estimate is -1.3% for a one-standard deviation assessment shock. The negative sign confirms that the tax channel outweighs the anchoring channel. On average, positive assessment shocks lead to lower prices and negative assessment shocks to higher prices. The event study regressions therefore confirm the base case results for the DD regressions in sign and order of magnitude.

Panel B of Table 5 analyzes the assessment uncertainty channel. In contrast to the assessment shock, which is only included in sales prices after AV publication, the uncertainty should affect transaction prices before publication. To provide a direct test of this prediction, we interact the tax-adjusted assessment uncertainty with a “Pre” dummy, which equals one if a transaction was made before publication, and zero otherwise. For all three models, the coefficient estimate is negative and significant. In column (3), which includes the most detailed control variables, the estimate is at -0.5% , i.e., a one-standard deviation change in the tax-adjusted volatility leads to a 0.5% decrease in sales prices. The event study regressions yield thus further support for the assessment uncertainty channel.

Panel C of Table 5 shows results for the additional controls included in the analysis. Unsurprisingly, past year’s log-AV predicts a large share of future price changes. The one-year AV growth rate yields additional explanatory power for the regression. Naturally, a higher assessment

Hence, the Baum-Snow and Han (2023) housing supply elasticity estimates used in our analysis, which appear to be primarily based on periods of price appreciation, may not fully capture this structural asymmetry.

Table 5
Supporting evidence: Event study regressions on log sales prices.

	(1)	(2)	(3)
<i>Panel A: Assessment shock</i>			
Assessment shock	-0.010 (0.007)	0.001 (0.010)	-0.029 (0.021)
Post × Assessment shock	-0.015*** (0.003)	-0.014*** (0.003)	-0.013*** (0.002)
<i>Panel B: Assessment uncertainty controls</i>			
Tax-adj. vola	0.012 (0.010)	0.005 (0.006)	-0.000 (0.010)
Pre × Tax-adj. vola	-0.010*** (0.002)	-0.006*** (0.002)	-0.005** (0.002)
<i>Panel C: Other controls</i>			
Local assessment volatility	-0.011 (0.010)	-0.005 (0.005)	-0.004 (0.009)
Local effective tax rate	0.026 (0.029)	0.004 (0.009)	0.012 (0.010)
One-year AV growth rate	0.097*** (0.006)	0.084*** (0.009)	0.120*** (0.024)
$\ln(AV_{i,t-1})$	0.829*** (0.011)	0.829*** (0.011)	0.817*** (0.008)
Municipality fixed effects	Yes	Yes	No
Zip-code-tax-year fixed effects	No	No	Yes
Tax-year-quarter fixed effects	No	Yes	Yes
Adj. R-sq.	0.925	0.925	0.929
Observations	152,783	152,783	152,783

This table provides regression results for three separate regressions with log sales prices as dependent variable. The regressions use only treated units, i.e., ones in reassessing municipality-tax-year clusters. Panel A shows coefficient estimates related to assessment shocks. “Assessment shock” is the unexplained variation of homes’ one-year AV growth rates and stems from a regression of AV growth on a set of zip-code-tax-year dummies, as well as municipality-interacted one-year (log) AV lags. A detailed description and the corresponding regression equation is provided in Appendix D. Panel B shows results for three regressions corresponding to the assessment uncertainty channel. “Tax adj. vola” describes the product of the “Local assessment volatility”, the municipality-tax-year dependent standard deviation of one-year AV growth rates, and the “Local effective tax rate”, the median effective tax rate per municipality-tax-year cluster (imputed, if necessary, by its one-year lag). Effective tax rates describe tax amount paid in a given year, divided by the corresponding AV. Panel C displays coefficient estimates corresponding to the control variables given in row names. “One-year AV growth rate” describes the contemporaneous relative change in the assessed value of a given home relative to its AV. “ $AV_{i,t-1}$ ” is the AV of a given home from the past year. All continuous variables are standardized to have zero mean and a standard deviation of one. Standard errors are clustered over counties. *, **, and *** indicate significance at the 5%, 1%, and 0.1% levels, respectively.

tends to be associated with a higher sales price. Notably, the local assessment volatility and the local effective tax rate (i.e., the two variables that yield the tax-adjusted volatility measure) are statistically insignificant. This is again in line with the model in Section 2, which also finds only the interaction of both variables to affect the equilibrium house price.

6. Identification

In this section, we provide additional support for the validity of our results presented above. In particular, we discuss our identification strategy in more detail. In Appendix F, we demonstrate the robustness of our results with respect to various specifications.

A potential issue could be that homeowners wait for publication until they sell their homes. Fig. 3 shows the frequency of transactions relative to AV publication for both treated (Panel A) and non-treated units (Panel B). For the treatment group, no sudden change in the transaction amount is evident around zero, indicating no immediate effect of AV publication on liquidity. With an increasing number of days after the publication date, however, there is an increase in observation frequency. Similarly, an increase in transactions is observed for the

control group, as shown in Panel B. For the control units, the increase is even stronger, suggesting that upcoming reassessments do not play a role when considering the timing of a sale. A potential reason for the observed increase over time for both groups is likely to be that AVs are mostly published in spring, and turnover is typically highest in summer. Additionally, waiting for AV updates should not be a concern in the housing market, as it is costly, due to factors such as interest payments and opportunity costs.

Another concern that needs to be discussed is that homeowners are able to challenge their assessment once they received notice of their updated AV. While data on changes of preliminary assessments is not available, it is possible to investigate how frequently tentative AVs are changed by utilizing 2016 data provided by the New York City (NYC) government.¹⁹ Yet, as the filing of the tentative roll provides the first opportunity for NYC-homeowners to contest their AV for the upcoming tax year, the NYC data should still inform about the frequency of successful contests for other municipalities in the state, in which the first possibility to review AVs exists after the mailing of preliminary AVs. Investigating the subsample for single-family homes (tax class 1), we find that only about 6000 AVs have been updated after initial mailing of AVs to homeowners. This accounts for only about 0.9% of the about 697,000 homes that are listed in the following year’s tax records in the *CoreLogic* dataset, suggesting that the possibility to contest public estimates is not made use of very often – presumably due to the risk of getting an even worse assessment. It affects only a small share of observations and should therefore be of minor relevance. Additionally, AVs are not meant to be forecasts but have to reflect home values at a particular date in the past (the “valuation date”), further supporting the causal interpretation of the results.

Finally, spillovers across municipalities should not be a concern, as homes are individually affected, depending on whether the updated AV is relatively high or low. That is, within a municipality, some homes can be positively, and some negatively affected. As this implies no shift in the aggregate price level in a particular direction within a municipality-tax-year cluster, no spillover effects across units due to AV publication should be expected.

7. Conclusion

Property taxes are commonly based on assessed values (AVs) of homes which can only be estimated, and are thus prone to valuation errors. This paper analyzes whether AVs themselves affect the trading prices of homes. Theoretically, the impact of AVs on transaction prices should be driven by two opposing channels. AVs constitute salient reference prices for market participants. Through this anchoring channel, there should be a positive causal effect of AVs on transaction prices. At the same time, an unexpected increase in the AV implies a higher future property tax burden associated with the underlying home. Thus, through this tax channel, a negative effect of AVs on sales prices should be observed.

We analyze this ambiguous relationship with a difference-in-differences framework that exploits AV-publication dates and variation in reassessment frequencies. The results document a robust negative causal impact of AVs on transaction prices, in line with the tax channel. With an AV-induced change of about one percent in the transaction price of the average home, the results of this paper are not only statistically robust, but also of economic significance. Supporting the tax channel as the driver of our findings, homes associated with high tax rates are affected more strongly than homes related to lower tax rates. Disentangling the aggregate effect into the tax and the anchoring effect, we do not find empirical support for the anchoring effect to

¹⁹ Available from the NYC Open Data portal at <https://data.cityofnewyork.us/City-Government/Revised-Notice-of-Property-Value-RNOPV-/8vbg-zm6e>, last accessed on March 22, 2023.

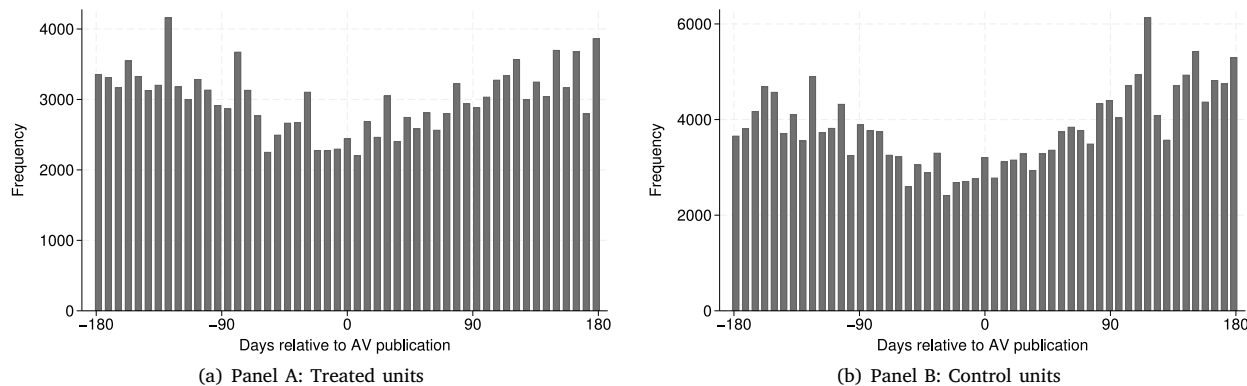


Fig. 3. Number of sales relative to the event date

The histograms displayed in Panels A and B show the number of observations in the dataset relative to their respective event dates within a time window of ± 180 days. Panel A shows the frequency for treated units, i.e., units located in reassessing municipality-tax-year clusters. Panel B shows the same variable for control units, i.e., those in which AVs remain collectively unchanged (zero-change) or adjusted by the same constant (constant-adjustment).

play an important role. That is, our aggregate treatment effect should essentially reflect the tax effect. The results documented in this work are robust with respect to different subsamples and fixed effects. The findings of this work have implications for the redistribution of wealth through the property tax system. That is, imperfect assessments not only lead to inequitable tax payments, but also adversely distort the transaction prices of homes.

While our work focusses on the implications of assessments for homeowners, they should also have implications for renters to the extent that property taxes are passed through to them in the same fashion as maintenance, insurance, and the like. Our results do not provide support for the existence of a strong anchoring effect, from which only homeowners would be directly affected. Increases in AVs should therefore affect owner-occupiers and renters in a similar fashion as higher assessed values should lead to a higher property tax burden for both owner-occupiers, that are directly affected, and renters, that are indirectly affected, via the pass-through.

CRediT authorship contribution statement

Marcel Fischer: Writing – review & editing, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Patrick Hauf:** Writing – review & editing, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Simon Stehle:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jbankfin.2025.107610>.

Data availability

The authors do not have permission to share data.

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